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THE FLYING-FISH PROBLEM[1]

LIEUT.-COLONEL C. D. DURNFORD

In a paper published in the *Annals and Magazine of Natural History* for January, 1906, the impossibility, from a mechanical point of view, of a flying-fish accomplishing sailing flight was shown. The argument was based upon the fact that as a flying animal the flying-fish is equipped with wings of a fractional sailing value compared with those of a sailing bird. Also that if the wings were many times larger, so as to bring the fish on an equality with the bird in this respect, it could only sail with the bird's limitations as regards direction of the wind, and with the bird's frequent assistance from rowing flight. Also that if the figures (which can be easily verified or, if wrong, refuted) are correctly given in the article, the accepted aeroplane flight is miraculous, unless a new law of Nature be discovered.

It is, then, perhaps advisable, if the present curious condition of the question is to be understood, to examine how it has come about.

The flying-fish problem is a very odd one in many ways, of which the most striking is the unexplained power therein of the negative to quench the positive. Throughout we find the aeroplanist's "I cannot see the wing-movement" smothering a fairly equal bulk of "I can, and have, and do see it."

Let us create a parallel instance, for a real parallel does not perhaps exist: — Many people can see bullets in their flight. Many others with equally good, or even better, sight cannot pick up the flying bullets. Now if those who fail to see them said, and if all books and papers on shooting supported them in so saying,

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[1] This article was intended to appear simultaneously in the *American Naturalist* and in the Annals and Magazine of Natural History but delays in the mail prevented. The subject however is of such interest that its later publication here may be pardoned.—Editor.

"I cannot see the bullets, therefore you, and all those who do see them, do not see them," we should have a parallel to the current odd mode of conducting the flying-fish problem.

It is in consequence of this supremacy of the negative that the flying-fish problem has earned for itself the name of "eternal," for as soon as one new witness can see the flight, either another new one fails to do so, or a reference is made to some observer who has formerly so failed; and this is equally satisfactory, for, in the problem, even an old "I did not" is better than a new "I do."

It might naturally be supposed that there must be an overwhelming backing of probability, both mechanical and natural, to the negative evidence in order to justify such dogged denial to the affirmative of its common value. So far, however, from this being the case, it is a second odd fact that but one seemingly practical effort at proof has been made, and with this one exception aeroplane flight rests wholly upon the flat negative.

Let us examine this solitary attempt at proof.

I requote from an article, which may be taken as typical of the system, in the 'Annual Report of the Smithsonian Institution,' 1904, p. 498, by Dr. Theodore Gill, an emphatic aeroplanist: — "Möbius (1878, 1885) contended that 'Flying-fish *are incapable of flying* [the italics are his], for the simple reason that the muscles of the pectoral fins are not large enough to bear the weight of their body aloft in the air.'" If undisputed that is, without doubt, a most powerful argument — decisive, in fact. But mark! almost immediately Prof. Whitman, a high authority, denies its accuracy. In the same article we find that this statement is "vigorously objected to by C. O. Whitman (1880), who urged, 'Admitting that in form, size, length, and structure the pectoral fins of *Exocœtus* are less well adapted to flight than the wings of most birds, there is still ample room to believe, on anatomical and physiological grounds alone, that they are capable of executing true flight.'" This is a plain statement moderately worded by a distinguished physiologist and naturalist, and it is interesting to note that it is answered, as though by convincing argument, by the old irritating *impasse* — the reference to views of distinguished naturalists as to whether flying-fish fly or do not fly, and entirely ignoring the new muscle aspect opened by Whitman.

Among the distinguished naturalists thus referred to in support of Möbius's theory, Prof. Moseley, as being of the 'Challenger' Expedition, and Mr. Boulenger are prominently mentioned. But Moseley, who cannot see the *Exocœtus* flapping, can see the Dactylopterids doing so (p. 512): the possibility of which act is denied by Möbius from personal observation as strongly as in the case of *Exocœtus!* Whilst Boulenger merely quoted the verdict of others, he himself retained, then as now, as he informs me, an open mind upon the question.

It is surprising how largely this "general verdict" is influenced by the researches of Möbius, the very Professor whose solitary so-called proof is questioned by Whitman; so we will examine more closely what he says about the muscles. The quotation is continued from "'aloft in the air,'" above.

"'The pectoral muscles of birds depressing their wings weigh, on an average, one sixth of the total weight of the body, the pectoral muscles of bats one thirteenth, the muscles of the pectoral fins of flying-fish only one thirty-second.'"

If this proves anything — which to the purpose it does not — it may prove that, as flying-fish have somewhat less than half the comparative muscle of bats, and (according to aeroplanists) cannot, for this reason, fly, therefore bats, which have somewhat less than half the comparative muscle of birds, cannot fly.

Or, the other way about: — Birds can fly. Bats, having rather less than half the comparative muscle of birds, can fly; therefore flying-fish having rather less than half the comparative muscle of bats, may fly.

Those are reasonable deductions, but "therefore flying-fish cannot fly" is an unreasonable one.

It is quite clearly a question of degree, and the true deduction is that bats, if they can fly, cannot be expected to fly like birds, and flying-fish, if they can fly, cannot be expected to fly like either bats or birds; and, I may add, no one thinks or claims that they do so fly.

But an even greater claim is made by aeroplanists. It is recognized that there are two kinds of bird-flight, "sailing" and "rowing," the sailing being greatly the superior form. Sailers can always row, but rowers cannot properly sail on account of their low wing to weight ratio.[1] Now flying-fish have a ratio of the lowest class in comparison with birds (see 'Annals,' Jan. 1906, p. 162); yet they are credited by aeroplanists with sailing of a higher form than that of the best-equipped sailing birds — sailing, without even occasional rowing assistance, at a slow speed, regardless of the direction of the wind ! Such a feat — one utterly impossible for an albatross,[2] an eagle, a vulture, kings of flight — is given to this last poor dabbler in the art upon persistently contradicted negative evidence, two impossible parallels, and the one discredited proof.

I have endeavored in the foregoing to show how observers have been weighted and clogged by the unique system of handling an admittedly difficult question — how a very able man, Prof. Möbius, years ago undertook a research which required a very special knack of eyesight in the observer. Probably the majority of men are without this knack, and do not know it. Firmly believing what I have endeavored to show must have been the false view presented to his retina, to be a true view, he wrote, with the cleverness that belonged to him and the dogmatism of the believer, the text of the faith which has guided and misguided scientists for over a quarter of a century. His reputation was, and is, deservedly great — so great that his word was practically law, and it came about that if other scientists possessed the knack of sight and differed from him so much the worse for them; they must be either ignored, or explained away, any or no explanation being sufficient for such a proper purpose. This is not a hard judgment. Anyone, who is free from the superstition, on reading an ordinary aeroplane article will recognize its justice.

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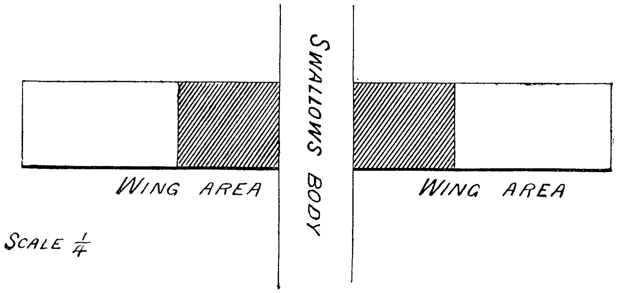
[1] Harting's formula $\frac{\sqrt{\text{wing-surface in sq. cm.}}}{\sqrt[3]{\text{weight in grammes.}}}$, which governs this ratio in birds, is impugned by R. von Lendenfeld in the volume that we have been quoting from (Ann. Rep. Smith. Inst. 1904, p. 129). The figures of his example in proof will not, however, bear examination. Correctly calculated they strongly support Harting $\left(\frac{\sqrt[2]{336}}{\sqrt[3]{320}} = 2{\cdot}68\right.$, and *not* 4·03 as given by Von Lendenfeld as the ratio of the partridge$\left.\right)$.

[2] Some notes by Prof. Moseley ("Notes by a Naturalist on the 'Challenger," p. 571, 1874) upon the small amount of true soaring performed even by the albatross are instructive. Our eyesight misleads us again in this matter.

Take a quite typical example of the common aeroplane blindfold acceptance from writer to writer of palpable impossibilities as guiding facts. In the article that we have been quoting from we may note the following (p. 500): "The best estimate has been that an ordinary flight may extend from 30 to 50 yards in less than twenty seconds." In order to get working figures we may call "30 to 50 yards" 40 yards, and "less than twenty seconds" 15 seconds. This gives a rate of 5½ miles an hour!

Note this, you who watch the fish fleeing before a 14-knot steamer.

Such statements are the habit of the problem. Just in the same way is it its recognized habit to quote, unquestioned, as "sailing" parallels to the heavy small-winged fish, the ¾-oz. large-winged swallow, and the parachute whose work is falling only; or, again, to faithfully reproduce over and over again pictures of impossible air-currents performing feats also impossible; or to continue to ascribe the frantic efforts at flight of a fish fallen on deck to natural spasms, although it is not credited with active use of its wings either in air or sea; and so on. It is the way of the problem, and no one is to blame.



Perhaps the odd unsuitability of the swallow comparison may be brought more fully home by a sketch.

The ratio (Harting's formula) of a swallow (house-martin) is 4.2, and its wing-area 120 sq. cm. The flying-fish ratio is 2.6. If we reduce the swallow to a 2.6 ratio, its wing-area becomes about 47 sq. cm.

This reduction to flying-fish ratio is shown by the shaded parts of the sketch.

Could anyone contend that a swallow could sail even in its present poor and much-assisted way (for it is far from being a first-class sailer) if the unshaded parts of the wing-areas were removed?

Opinion is, however, undoubtedly changing. Many of the old shibboleths are fast becoming discredited. The great distances that the fish, under favoring conditions, fly clear of the water[1] — the fact that they fly in calms as in winds — that they come on board ships from lee and weather sides indifferently — that they can and do turn in air[2] — that they often lose and often gain speed, both from simple causes, on meeting a wave or on tail-dipping — that they can and do at times gain speed whilst still in air — that they make for lights deliberately — that they rise and fall of set purpose while in the air: all these and much more that has been under the ban are being witnessed and certified to so incessantly that soon only the high-priests of aeroplane will be left contradicting them.

F. G. Aflalo ('Natural Hist. of Australia,' Macmillan & Co., 1896) writes: "I have watched these beautiful creatures by the hour and in all weathers, ... but after having closely watched thousands of them through strong glasses, I cannot give as emphatic an opinion as I should like on the oft-discussed question of whether the wings vibrate like those of birds.... If the pectoral fins are so constituted as to be capable of vibration, then I would say as the result of my own observations that to some slight extent they do flap, not like those of birds, perhaps, certainly not like those of the bat."

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[1] It is difficult to judge distance at sea. The tendency is to underestimate it. Many observers testify to having seen flights of more than a quarter of a mile. Frank Bullen, in his article upon flying-fish in 'Creatures of the Sea,' insists that he has seen flights of over a mile. He has had exceptional opportunities for observing them, and I see no reason for thinking that he is mistaken.

[2] With reference to their turning powers. I mentioned in the former paper a fish which I had seen to turn back in air. I then restricted myself to the bare facts required for the argument. It had interested me, however, much at the time, not only because it was, to me, a rare occurrence, but also because the controlling cause of the fish's remaining and turning in air was quite evident. The sea was rather calm and the ship was throwing out, with each gentle roll and dip, those broad hissing tables of white foam which spread away for many feet from her sides, and die in a mass of struggling bubbles, to reappear as the white broad rushing table of the next dip. The fish had risen independently of the ship, and was flying towards us at full speed, when a sudden slow down marked its perception of the advancing monster. There was no time, however, for it to decide whether water or air was the less perilous before it was over an unusually broad table of boiling foam. The hidden and fearful possibilities of this evidently decided it, and then ensued its slow but successful struggle to turn and get clear of the concealed horrors. This it did with what must have been a terrific effort, but it got quite round and well away out into the blue water before it dived.

I have quoted the above as it expresses markedly two common difficulties: (1) the real difficulty in discerning the movements; (2) the pre-acquired idea that the wings are not fitted for flapping, an idea which naturally greatly increases difficulty (1). Had Mr. Aflalo been certain of the two facts that the wings were fitted for flapping and that "sailing" was for the fish ordinarily impossible, it cannot be doubted that his views would have been stronger and expressed very differently.

Among quite recent papers upon this question, two should be especially noted. Lionel E. Adams, B. A., writes in the 'Zoologist' (April 4th, 1906) an article interesting throughout. I quote from p. 146: ".... I was often able to see them against the sky.... I could see quite distinctly that their tails were vibrating very rapidly from side to side during the whole flight, and that the wings would vibrate with an intensely rapid shivering motion for a second, then remain outspread motionless for one or two seconds, and then vibrate again. This vibration of the wings is not up and down as in the case when birds fly, but in an almost horizontal direction."

That is a quite possible explanation of the mode of flight, provided that a sufficient speed be acquired in the intermediate flappings, but this the known speed of the fish shows to be not commonly the case.

Again, on p. 148: "I am perfectly well aware that a casual glance at flying-fish from the lofty deck of a liner gives the impression that they soar like birds with motionless wings, but watch them at close quarters from the deck of a low-waisted tramp and the vibratory motion of the tail and fins will be quite plain."

Interesting as is Mr. Adams's paper, I cannot but think that he is partly mistaken in his views, and that the wing-vibration which he discerned was really less rapid than the movement in the period following which he believed to be one of stillness, just as the line-passengers mistook his vibrations for stillness. I do not say that the fish could never arrive at a speed by which a very short aeroplane flight could be attained even with their low ratio; but I do say that such is not their common speed, and that in any case their disregard of wind-direction disproves such flight.

Therefore another way must be looked for, and we are driven back, perforce, to continuous wing-action, the manner of which may be here examined as carefully as our information allows.

Premising that the flight varies greatly on different days and under different conditions, the following is probably a fair description of their methods in an ordinary flight:—

1. The tail-impelled, visibly (to many) wing-assisted jump from the water to a height where the wings can work freely.
2. The flight continued by an intensely rapid and labored wing-movement — one easily mistaken for stillness, and usually seen, if at all, as blur.
3. Short periods of slowing down of wing-speed, during which the wing-movement becomes again visible. (These are the "vibration" periods, representing to aeroplanists loose wing-trailing, or dragging like a flapping flag—an impossibility; and, to Mr. Adams, periods of wing-assistance — with limitations a possibility.) These periods often precede a special spurt such as is required to lift the fish over an oncoming wave.
4. Either sudden cessation of wing-movement and consequent immediate drop into the sea or a short slow down into visibility (No. 3) previous to such drop.

It is to be noted that this vibration so often seen before the fish enters the water is one of the many pointers to continuous wing-movement, for such a time is a proper one for slowing down, but an absurd one for renewal of wing-effort.

To return to Mr. Adams's paper. He notes, as have others, the vibration of the wings as being in "an almost horizontal direction." This horizontal movement, if it exists, as is probable, may afford, as I hope to show, a looked for key to the fish's action.

According to Pettigrew, it is a necessity of flight, where wing-beats are in a more or less vertical direction, that the up-beat should meet with little and the down-beat with much resistance from the air. This is arranged for in the case of bats, birds, and certain insects by means of special muscles and ligaments which automatically flex the wing for or during the up-stroke, and extend it for or during the down. (Pettigrew, 'Animal Locomotion,' Int. Science Series, vol. vii. pp. 122, 182, 194, &c.: 1891.)

Marey ('Animal Mechanism,' p. 263 &c.: Int. Science Series, 1893) equally recognizes the necessity for a diminished wing-area in the up-stroke, but believes it to be obtained in birds through the natural elasticity of the feathers, which enables them to return to their ordinary position when the resistance of the air in the down-stroke ceases to raise them.

The flying-fish's wing, as is known, is formed on quite a different principle from that of a bird or bat. It opens and closes somewhat like a fan. A partial automatic closing of this fan at the foot of the downward stroke in flight and opening at the top of the rising stroke would both give the appearance of horizontal vibration when seen either from above or below, and would turn a somewhat difficult question of the mechanics of the flight into a very simple one. Indeed we have here flying action on the same general principle as that shown by Pettigrew and Marey to be necessarily provided for in the case of bats and birds, but the working details of which are different and simpler, as becomes a simpler form of wing.

Perhaps that is the explanation. There must, of course, be some explanation, and that is not only the natural deduction from the peculiar formation of the wing, but it also fits everything in.

The known (but indistinct) visibility of the larger rays of the wings at times during flight points, perhaps, to a comparative pause with wings full open before beginning the down-stroke. Such pause would give the open position, and with it the wing-tracery prominence.

The form of these fishes' wings points to this fan-action rather than to other known horizontal wing-actions of the nature of that of certain insects — the common fly, for instance (Marey, *loc. cit.* pp. 204, 206).

The second quite recent and very important observer and writer on this subject is convinced of the flight-action. He writes also from personal observation, and is as free from proper mechanical bias as from the improper follow-my-leader habit. One of his remarks, "It is by no means impossible that flying-fish may soar, as *even* [my italics] birds do this," shows his mechanical freedom. In a paper dated Oct. 28th, 1905, Brig 'Galilee,' North Pacific Ocean, Dr. J. Hobart Egbert, Carnegie Expedition, writes ('Forest and Stream,' Jan. 27th, 1906): "Though still denied by some observers, the power of propulsion through the air by means of its fin-wings is generally accorded the flying-fish.[1] During months at sea in the tropics the writer has almost daily watched the flying-fishes and studied their flight through the air.... The difficulties of assuring oneself that the flying-fish moves its wings during its flight through the air are well understood, and also the fact that these difficulties are generally removed when opportunity is afforded of observing the flight of certain of the larger species under favorable conditions. That flying-fishes use their wings after the manner of birds, at least upon emerging from the water, can hardly be denied, since from the fo'c's'le head of a ship plying the waters of the lower latitudes this wide bird-like motion of the fin-wings may be easily observed as the large flying-fishes break water almost under the vessel's bow. This flapping motion of the fin-wings is not, however, long maintained, but as soon as the fish is well started in the air apparently passes into a vibratory motion of the appendages so rapid as to be almost beyond human visual perception."

Quite so. That is the to-be-expected flight of an exceptionally low-ratio flyer having special added natural disabilities. Before long it will be the accepted one for flying-fish.

More about the Pectoral Muscles.

Since writing the foregoing I have received a communication from Prof. C. Stewart, F. R. S., Conservator of the Museum of the Royal College of Surgeons, who kindly gives me permission to use the results of a dissection made at the Museum for the purpose of comparing the pectoral muscles of the flying-fish with those of a nearly related non-flying fish.

I quote from the letter of Mr. Burne, who made the dissection:—

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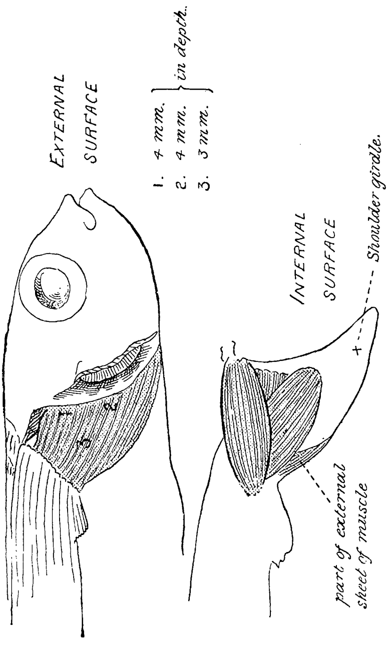
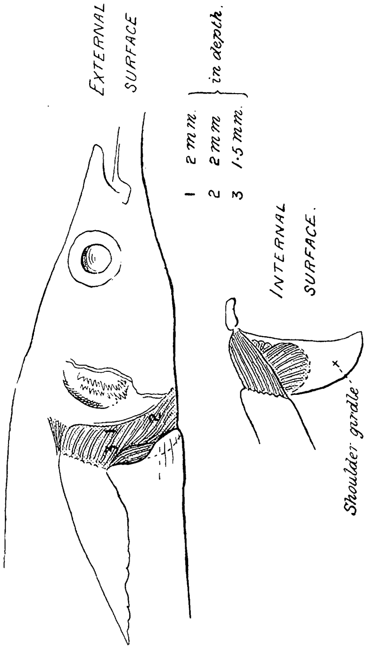
[1] A little premature, if Natural Histories and Encyclopædias are any indication of general accord.— C. D. D.

"Royal College of Surgeons of England,
Lincoln's Inn Fields,
London, W. C., 18th June, 1906.

"Dear Sir,— . . . . I have made a dissection of the pectoral muscles of a flying-fish (*Exocœtus* sp.) and of a nearly related fish of much the same build, but without the enlarged pectoral fins (*Hemiramphus*). Both were specimens from our store-room, and although in pretty good condition had evidently been in spirit for a considerable time. I enclose you tracings of the drawings I made. The two of the external view were drawn with a camera, and the *Hemiramphus*, which was rather less in girth than the *Exocœtus*, was so much enlarged as to have the same girth about an inch behind the pectorals. I thought that body-girth sufficiently far behind the fins not to be influenced by their degree of develop-



Exocœtus



Hemiramphus

ment was the best standard of size to take — better than length, for instance. As a matter of fact, the fish were very much the same length; the *Exocœtus* being rather the longer.

"The drawings, I think, explain themselves. The flying-fish muscles were, as you see, considerably larger, both in area and in thickness, than in *Hemiramphus,* and the same was the case with the muscles on the deep surface of the fin. In their arrangement they were much the same in both fish and the same as in other bony fishes (the cod, for instance). The numbers on the surface of the fins are the points where I took the thickness of the muscles by plunging a needle into it and measuring the depth to which the needle entered. You will notice the great length of the muscles in *Exocœtus*: a long muscle means a proportionate length of contraction.

" . . . . there is a very marked difference in the size of the muscles of these two fishes. . . .

"Believe me, yours faithfully,

R. H. Burne

(*Assistant in Museum*).

The above tracing seems to give, roughly, about $4\frac{4}{5}$ times greater bulk of muscle to the *Exocœtus* than to the *Hemiramphus.* With this light it will not be out of place to requote and amplify the one "proof," distinguishing the addition by italics: — "The pectoral muscles of birds depressing their wings weigh on an average one sixth the total weight of their body, the pectoral muscles of bats one thirteenth, the muscles of the pectoral fins of flying-fish. . . . one thirty-second," *and the muscles of a nearly related non-flying fish only one hundred and fifty-fourth.*

As before, it does not prove that bats or flying-fish flap or do not flap their wings, but it gives a different and, I hope, a proper aspect to the figures which have done duty — of a kind — for so many years.